read during the session 1871-2, some of which are already known to our readers. We need only name the principal papers. There is, first, the Presidential Address of 1871, "On Motive Power," delivered by Mr. J. J. Murphy, who has also a short paper on "The Bernina Lakes;" then comes Prof. James Thomson's admirable paper, "Speculations on the Continuity of the Fluid State of Matter, and on Transitions between the Gaseous, the Liquid, and the Solid States." This is followed by two short papers, one by Dr. J. D. Everett on "The Reduction of Observations of Wet and Dry Bulb Thermometers," and another on "Recent Changes of Coast-level at Ballyholme Bay, Co. Down," by Mr. Robert Young, C.E., who has also an excellent paper on "The Duty of Preserving National Monuments." Mr. John Anderson contributes a paper on "The Geological Formation of County Down," the Rev. Dr. Macloskie a long paper on "The Silicified Wood of Lough Neagh;" and there are also one or two papers of antiquarian and social interest. Appended is an interesting obituary notice by the secretary, Mr. Taylor, of the late Mr. Robert Patterson, F.R.S., one of the founders of the Society, and who, amid the cares attendant on the carrying on of a large commercial establishment, managed to find time to prosecute to very good purpose the study of natural history, and even to write admirable zoological text-books, and take an active part in the promotion of science and of social progress. The first number is edited by Mr. Murphy and Dr. H. Burden, and we hope the Society will produce material enough to bring out an equally good number every year.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Dr. Huizinga's Experiments

In a letter published in last week's Nature, in which Dr. Bastian comments on a short paper read by me at Bradford on certain experiments of Dr. Huizinga, he challenges me to deal with his "main proposition," which is "that Bacteria are capable of arising in fluids independently of living reproductive or germinal particles."

In so far as relates to the subject of my communication, I have done so by showing that in the case of Huizinga's liquid Bacteria can be prevented from arising by heating the liquid to a temperature somewhat above boiling.

I hope that Dr. Bastian will allow me to decline to enter on the general question, and will believe that in doing so I am not insensible either to the difficulties of the subject, or to the value and importance of his own experimental investigations.

Oct. 13 J. Burdon Sanderson

Experiments on the Development of Bacteria in Organic Infusions

The correspondence in your journal on this subject (relating chiefly to the statements of Dr. Bastian) in which I took a part some six or seven months since—renders it necessary, in justice to myself, and I may add, in justice to the memory of my friend, Dr. Pode, whose loss has prevented me from continuing a series of experiments on the nutrition of Bacteria, commenced in the spring—to give some account in your columns of experiments carried out by us, which demonstrate that Dr. Bastian's assertions as to infusions of turnip and turnip-cheese are devoid of foundation in fact. The paper in which our results are given in detail was sent in to the Royal Society at the end of last March and printed in May (Proceedings, No. 145). Since you are not able to afford space for the reproduction of that paper in full, I must beg to refer your readers, for details, to that publication of the Royal Society. Here I may be allowed to sketch briefly the results and their bearing on Dr. Bastian's statements. The following passage from that gentleman's "Beginnings of Life" (vol. i. p. 429) induced us to make experiments similar to those mentioned in it, with the view of testing the correctness of his conclusion as to matter of fact:—

"On the other hand, the labours of very many experimenters have now placed it beyond all question of doubt or cavil that living *Bacteria*, *Torulæ*, and other low forms of life will make their appearance and multiply within hermetically-sealed flasks (containing organic infusions) which had been previously heated to 212° F., even for one or two hours. This result is now so easily and surely obtainable, as to make it come within the domain of natural law." And in a note is added, "in a very large number of trials I have never had a single failure when an infusion of turnip has been employed; and from what I have more recently seen of the effects produced by the addition of a very minute fragment of cheese to such an infusion (see Appendix C, pp. xxxiv.-xxxviii), I fully believe that in 999 cases out of 1,000, if not in every case, a positive result could be obtained."

The extract which follows is from a paper by Dr. Bastian in Nature, vol. vii. p. 275, and is perhaps more remarkable than the preceding, because it is of later date and refers to a simple infusion of turnip.

"Taking such a fluid, therefore, in the form of a strong filtered infusion of turnip, we may place it after ebullition in a superheated flask, with the assurance that it contains no living organisms. Having ascertained also, by our previous experiments with the boiled saline fluids, that there is no danger of infection by *Bacteria* from the atmosphere, we may leave the rather narrow mouth of the flask open, as we did in these experiments. But when this is done, the previously clear turnip-infusion *invariably* becomes turbid in one or two days (the temperature being about 70° F.), owing to the presence of myriads of *Bacteria*." The italics are my own.

Dr. Pode and I give in our paper the details of 53 experiments, of which 11 were made with hay-infusion, the rest with turnip- or turnip-and-cheese infusion. We had some trouble at first in ascertaining some of the conditions under which Dr. Bastian experimented—since he does not state them in his book In the first place we ascertained through these columns the specific gravity of Dr. Bastian's turnip-infusion. We made a number of experiments after obtaining that information, which are recorded in our paper, and which invariably gave opposite results to those obtained by Dr. Bastian. At the beginning of this year we ascertained through Dr. Sanderson, in the columns of Nature, that Dr. Bastian made use of two-ounce retorts; and that particles of cheese visible to the naked eye were present in his infusions at the time of boiling. Dr. Sanderson also stated that Dr. Bastian attached importance to the peeling of the turnips used. With this additional information we made further experiments, which tend to explain the failure of Dr. Bastian to keep his infusions free from Bacterian contamination.

There are four points which require attention in these experiments, and which were attended to in our series, but we must suppose were not attended to by Dr. Bastian.

In the first place the infusions were examined by the microscope at the time of sealing the tubes, as well as subsequently. What we sought to determine was whether a change had occurred in the infusion. Spherical and other particles besides dead Bacteria occur in freshly-boiled infusions, which might lead to erroneous conclusions when seen subsequently, if their previous existence had not been ascertained.

Secondly, we employed small tubes, five inches in length and of half-inch bore. It appeared to us not at all improbable, from the results of some experiments made by us with retorts such as Dr. Bastian used, that a boiling for five or ten minutes, before closure, of an ounce of liquid in a vessel of that peculiar shape, might sometimes give a development of Bacteria, owing to the protective effect of "spluttering," and the large mass to be guarded.

Thirdly, in the majority of cases—though we had no reason to doubt the efficiency (as proved by the results) of the boiling for five minutes in one of our small tubes, *ceteris paribus*—yet, to ensure thorough exposure of every part of the tube and liquid to the boiling temperature, we submerged many of our tubes in boiling water for a quarter or a half an hour after their closure. This method we finally adopted as the most certain to ensure the destruction of Bacterian contamination; it is essentially the same as that subsequently adopted by Dr. Burdon-Sanderson in the experiments described in a letter in Nature, vol. viii. p. 141, the difference being that, "to make assurance doubly sure," Dr. Sanderson raises the temperature of the water in which his tubes

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are submerged above 100 C. by increasing the pressure under which ebullition is effected, beyond the normal atmospheric limit.

A fourth point to which we gave attention was the possible preservative effect of "lumps" on Bacteria or their germs. No one would have supposed that Dr. Bastian neglected the precaution of removing large particles of cheese from his experimental infusion. We always strained our cheese emulsion very carefully, or else filtered it. Prof. Cohn found that an infusion made by boiling a pea in water developed Bacteria when the pea was left in it; but if the pea were removed, and the infusion subsequently reboiled, no Bacteria were developed. We found that lumps of cheese could really act as protective hiding-places for Bacterian contamination. In a retort—similar in every respect to Dr. Bastian's—this result was first obtained, though other retorts similarly treated were barren. Accordingly we prepared twelve tubes exactly alike, with the exception that in six the cheese was added as an emulsion, in the other six in the form of lumps. The tubes were closed, and submerged in boiling water for five minutes. Of the "emulsion"-tubes, one burst in the boiling, the other five were barren; of the "lumpy"-tubes, four developed Bacteria in quantity, two remained barren.

In the experiments recorded in NATURE, vol. viii. p. 141, by Dr. Sanderson, it is shown that even when "lumps" are avoided, and the infusion heated by submergence in boiling water, this may not prevent the development of Bacteria when a large bulk of material is employed. But boiling for such a length of time as one hour, or heating to 101° C., always gave him a barren infusion. Dr. Sanderson does not believe that there is a definite relation between the precise temperature to which the infusion is exposed and the destruction of Bacterian contamination, but that the *longer* heating, or the heating to a *higher* degree, will increase the *chance* that Bacteria or their germs are destroyed. Further, Dr. Sanderson's results agree with those of Dr. Pode and myself as to simple turnip infusion. With this infusion I understand that he has not found the same length or amount of heating necessary as with the turnip infusion to which a fragment of cheese has been added.

And now, I wish very briefly to point out where Dr. Bastian's statements are affected by these results. It is necessary that this should be clearly and simply put, because I find that many persons are under the impression that the investigation of the grounds of Dr. Bastian's statements has shown that there was some solid foundation for them. This is, however, in my opinion, not the case. It is *not* "beyond all question of doubt or cavil that living Bacteria, Torulæ, and other low forms of life will make their appearance and multiply within hermetically-sealed flasks (containing organic infusions) which had been previously heated to 212° F. even for one or two hours." On the contrary, no organic nor inorganic infusion has been contrived by Dr. Bastian nor by anyone else which will develop Bacteria, still less Torulæ, after exposure for one hour (or even less) to 212° F. This is the conclusion given by the impartial examination of the subject, indicated in the experiments above quoted.

Moreover, the statement in the second quotation from Dr. Bastian is abundantly contradicted by the experience of Dr. Sanderson, Dr. Pode, and myself. Such a turnip-infusion, placed as directed by Dr. Bastian, does *not* invariably become turbid in one or two days, owing to the presence of myriads of Bacteria. We have often kept such infusions free from Bacteria for many days, and I preserved one in a retort with its beak inclined downwards for more than six months, clear as crystal, but amply capable of sustaining the life of Bacteria, as was proved by its accidental contamination a week ago.

It is my opinion that the only *positive* addition to knowledge which this inquiry about the development of Bacteria in infusions has led to is, that when you have cheese-emulsion, or similar material present in an infusion, you must be a little more careful about heating it than when you have not, if you wish to destroy by the agency of heat the life of Bacteria or their germs contained in the infusion. How it is that cheese-emulsion helps the Bacterian contamination to escape destruction we do not know. Possibly in the same way as the larger lumps do. But that matter remains for inquiry when more is ascertained as to the natural history of the Bacteria. I think we may now feel fully satisfied that "archebiosis" or "abiogenesis" is not in any way rendered more probable than it was before by Dr. Bastian's experiments with organic infusions. Prof. Smith and Mr. Archer, of Dublin—eminent authorities in the study of the lower algæ—have criticised in detail and suggested explanations of some of the statements in the third part of "The Beginnings of Life," viz., statements relating to the transformation of various species of organisms into others. They show (the reader may consult Prof. Smith's paper in the October number of the *Quarterly Journal of Microscopical Science*, 1873) that the asserted "facts" of transmutations are *not* facts. It is abundantly demonstrated that the fundamental observations recorded by Dr. Bastian are erroneous, and that he has been mistaken.

Exeter College, Oxford, Sept. 26 E. RAY LANKESTER

Variations of Organs

MY father finds that in his letter, published in your number for September 25, he did not give with sufficient clearness his hypothetical explanation of how useless organs might diminish, and ultimately disappear. I therefore now send you, with his approval, the following further explanation of his meaning.

If one were to draw a vertical line on a wall, and were to measure the heights of several thousand men of the same race against this line, recording the height of each by driving in a pin, the pins would be densely clustered about a certain height, and the density of their distribution would diminish above and below. Quetelet experimentally verified that the density of the pins at any distance above the centre of the cluster was equal to that at a like distance below; he also found that the law of diminution of density on receding from the cluster was given by a certain mathematical expression, to which, however, I need here make no further reference. A similar law obtains, with reference to the circumference of the chest; and one may assume, with some confidence, that under normal conditions, the variation of any organ in the same species may be symmetrically grouped about a centre of greatest density, as above explained.

In what follows I shall, for the sake of brevity, speak of the horns of cattle, but it will be understood that my father considers a like argument as applicable to the variations of any organs of any species in size, weight, colour, capacity for performing a function, &c.

Supposing then that a race of cattle becomes exposed to unfavourable conditions, my father's hypothesis is that, whilst the larger proportion of the cattle have their horns developed in the same degree as though they had enjoyed favourable conditions, the remainder have their horns somewhat stunted. Now, if we had made a record of the length of horn in the same species under favourable conditions, we should, as in the case of the heights of men, have a central cluster, with a symmetrical distribution of the pins above and below the cluster. According to the hypothesis, the effect of the poor conditions may be represented by the removal of a certain proportion of the pins, taken at hazard, to places lower down, whilst the rest remain *in statû quo*. By this process the central cluster will be slightly displaced downwards, since its upper edge will be made slightly less dense, whilst its lower edge will become denser; and further, the density of distribution will diminish more rapidly above than below the new central cluster.

Now, if horns are useful organs, the cattle with shorter horns will be partially weeded out by natural selection, and will leave fewer offspring; and after many generations of the new conditions, the symmetry of distribution of the pins will be restored by the weeding out of some of those below the cluster, the central cluster itself remaining undisturbed.

If, on the other hand, horns are useless organs, the cattle with stunted horns have as good a chance of leaving offspring (who will inherit their peculiarity) as their long-horned brothers. Thus, after many generations under the poor conditions, with continual intercrossing of all the members, the symmetry of distribution will be again restored, but it will have come about through the general removal of *all* the pins downwards, and this will of course have shifted the central cluster.

If, then, the poor conditions produce a *continuous* tendency to a stunting of the nature above described, there will be two operations going on side by side—the one ever destroying the symmetry of distribution, and the other ever restoring it through the shifting of the cluster downwards.

Thus, supposing the hypothesis to be supported by facts (and my father intends to put this to the test of experiment next summer), there is a tendency for useless organs to diminish and finally disappear, besides those arising from disuse and the economy of nutrition.

Down, Beckenham, Oct. 4 GEORGE H. DARWIN

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